

DIET IN DISEASE

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A few months ago I was asked to serve on a committee for revising the dietaries of a group of hospitals. We were faced with the limitations of a relatively small budget, a limited number of dietitians, and a large number of patients. The collection of special diets used in these different hospitals was quite formidable, but we found, somewhat to our surprise, that a great many of the indications could be met with a much smaller variety of diets. The urgent necessities of the situation focussed our attention on the fundamental principles underlying the prescriptions of diet in disease. With your permission, I shall treat this as a meeting of a somewhat larger committee whose chief purpose is to consider these principles. For purposes of discussion, we shall confine ourselves largely to hospital dietaries but everything that we say may be applied also to private practice. Some of you may be disappointed because I cannot tell you certain combinations of food that are ideal for certain diseases. I should like to be able to do this but it is difficult to predict how long any foods would be the proper ones. Fashions in diet change rapidly—fundamental principles remain valid for many decades.

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At the very start, we should consider the nature of the evidence and the strictness of the criteria that must be employed in making our judgments. Our customs regarding foods become fixed so rapidly and so firmly that it seems almost impossible to apply to them scientific methods. We are accustomed to three meals a day at certain recognized hours. The number of the meals and the hours are largely determined by chance, and, as every traveller knows, the number of meals and the hours vary widely in different civilized countries; even in the same country they have varied in succeeding generations. Primitive man probably got along very well with wide variations in his meal hours. Different races get along very well with enormous differences in food materials. Doctors who specialize in diabetes have been satisfied, at various times during the last two decades, with régimes that differ quite radically. Judging by our experience in diabetic and in infant feeding, we must recognize the fact that a diet universally accepted one year may be almost entirely abandoned in half a decade.

Where do we get our information regarding foods? A small portion of it comes directly from recognized authorities on nutrition—the largest portion of it from advertising agencies who have either broadcast this information accurately or in a form distorted to suit their purposes, or, indeed, they have made up their own misinformation in order to boost the sale of their products. I do not believe that the public has realized the cleverness of the modern food advertising agency. The men who write advertisements are very highly paid, they are extremely intelligent, and they have at their command every available source of information. They can spend months in the preparation of one short sentence which serves as a slogan, and in this they are encouraged by some of our greatest universities. There is no question regarding the efficiency of these advertisements, but as to their sincerity, we sometimes have grave doubts. If then the American public is to derive its information from advertisements, it is the bounden duty of the medical profession to employ

its powerful influence in directing this advertising. It was for this reason that about a year ago the Council on Pharmacy and Chemistry of the American Medical Association established a Committee on Foods. You are familiar with the work of the Council in its censoring with great care the advertisements of drugs and medicinal foods accepted for the book "New and Non-Official Remedies." The United States Government has accomplished a tremendous amount of good by establishing and maintaining standards with regard to foods and by insisting that the label shall contain an honest statement of the contents of a package. Unfortunately, the Government has not been able to control the advertising and some manufacturers have taken advantage of this freedom. Our Food Committee, therefore, has directed its attention almost exclusively to advertising. If a manufacturer wishes to have his food product accepted for an advertisement in the *Journal of the American Medical Association*, or in *Hygeia* and to have it included by the Committee in its forthcoming book on foods, he must submit a full statement of the composition of the food and its method of preparation, and must also present every scrap of his advertising. Claims are carefully examined by a referee and are then passed on by the Committee. The result is that foods with honest advertising are accepted promptly and are allowed to be stamped with the seal of the Food Committee. Promoters of foods heralded with nutritional or health claims that are doubted are requested to give proof of the veracity of their statements, or to alter the food, or the claims, to conform to the truth. Almost invariably the producers have complied with our requests and their products have been made acceptable to the Food Committee. Sometimes such an investigation has even necessitated a complete change in the name of a product. The Committee has tried to be broad-minded and lenient and has allowed certain statements such as "The most delicious food you have ever tasted." This epithet is harmless because it is so generally applied, and it probably does make the food taste better. On the other hand, the state-

ment "An ideal food" has been rigorously excluded, and all claims to high nutritive value or a high vitamin content have been held up until proper proof could be furnished. Claims of usefulness in particular diseases have undergone the most careful scrutiny. The Committee has probably made a good many minor mistakes, but the result is that you can believe the statements made in the food advertisements of the *Journal of the American Medical Association*, or in any of the advertising matter that bears the seal of the Committee.

Now if this can be done in the case of individual foods, why cannot we here at this meeting use the same standards for various diets employed in disease? I have already mentioned the fact that innumerable diets are used in different diseases. If you look on stack S 21 of the Academy Library you will find about fifty feet of shelf room devoted to books on diet. A few inches of space suffice to contain the books that deal with the general principles, while many feet are required to hold the lists of foods supposed to benefit certain maladies. How the authors make their fine distinctions remains a mystery. One firm advertises a hundred different lists designed to accommodate any disease known to man. Another book, and not a bad book at that, lists all sorts of diets. For example, on page 194 is found "Nutritious diet (for adult male stock broker under nerve strain)." Page 86, "Gastritis, chronic, alcoholic (travelling man)." Page 81, "Fattening diet (for frail young mother)." And the next diet, also a fattening diet, is intended for "Spinster with impaired digestion." I searched for the difference between the diet for the spinster and the frail young mother and found that they were identical except that the young mother is given, on retiring, "a glass of milk, three or four lady fingers, or bread and butter." Undoubtedly the most popular diet in the book is to be found on page 207, under "Obesity." "The object of this diet is to retain the figure of youth in an unoccupied woman past forty."

Let us consider critically certain factors that go to make

up a diet. First, we have the caloric content. How closely can we estimate this? Should we say that a given patient needs enough calories to cover his basal metabolism plus 5-10 per cent for the specific dynamic action of his food plus 20-100 per cent for his bodily activity? There is a great deal of guessing called for, and it is only the man who guesses well who will come any where near the correct answer. The estimation of the basal metabolism contains an error of 10 or 20 per cent, but this is dwarfed by the estimation of the allowance for bodily activity. There are large individual variations. In general, the figures given in the text-books for people of various sedentary occupations would probably apply to most of our ambulatory patients as satisfactorily as calculations built up from the basal. The question next arises as to the smallest differences in caloric values that can be appreciated by the patient's metabolism. I doubt if a change of less than 10 per cent is perceptible except in a very delicately balanced metabolism experiment. A patient receiving say 2,000 calories would probably not react to changes much smaller than 200 calories. In other words, if we were planning a diet we could make a jump from 2,000 to 2,200 without bothering about the intermediate steps. I heard the other day of a man who wrote a book on dietetics making his increases five calories at a jump. This reminds one of the little boy who asked if it would harm the elephant if he gave him a currant from his bun.

I do not see any reason why we should not make jumps of 10 or even 20 per cent in our protein ration. For amounts of protein of over 70 grams a day it is doubtful if increases of less than 20 grams at a step are perceptible in their effects. If, in our metabolism ward, we have a patient in very delicate nitrogen balance on a low protein intake and follow him for months at a time we can discern a few minor changes in his metabolism caused by changing his protein intake 10 grams a day. Just what the optimum protein intake is for any special disease remains very much in doubt. There is no point in restricting the protein intake below 30 grams a day even in the most severe

nephritis, because if the patient doesn't get this protein from his food he will get it from his body, and the effect on the metabolism and excretory organs will be just the same. There are some indications in nephritis with nitrogen retention, and in severe diabetes, for keeping the protein intake between 30 and 50 grams for a restricted period of time. For most diseases we do not really know where to set the protein intake, except that it probably should be somewhere between 50 and 150 grams a day. For growing children and for convalescents, and perhaps for certain patients with nephrosis, intakes higher than 120 grams may be indicated. We, probably, have been too much afraid of protein food and it is certain that a good many people tolerate liberal protein rations surprisingly well.

It is doubtful if our steps in the carbohydrate ration need ever be smaller than 10 grams at a time. Perhaps in a very carefully balanced diabetic patient observed in the metabolism ward with extreme care we can see some metabolic effect from the addition or subtraction of 10 grams of carbohydrate, but in the ordinary patient about 20 grams must be added before the effect is apparent. I wish we knew the minimum carbohydrate intake that is compatible with perfect health. Joslin and some of the other workers in diabetes are much concerned with the possible danger of producing arteriosclerosis by giving, for long periods, diets that contain less than 100 grams of carbohydrate a day. This may be important, but I am not acquainted with any adequate proof of the contention. We must remember that there are tribes of Esquimos that live long and contented lives on a meat diet with a total carbohydrate intake of 30 or 40 grams a day. There is no evidence that they suffer from arteriosclerosis or kidney disease any more than the rest of us. Certain it is that arteriosclerosis is common and of major significance among the poor of our city whose diet is particularly high in carbohydrate. I am rather surprised that no one has advanced the argument that a high carbohydrate diet predisposes to arteriosclerosis.

When it comes to fat, this substance is so inert and so easily deposited in the body, or lost from the body, that we have been able, in our metabolism experiments, to change the fat intake 100 or even 200 grams at a jump without any perceptible change in the metabolism. Of course, an excess of fat long continued may lead to a higher fat metabolism and eventually obesity, while a deficit if not supplied by calories from other sources will result in undernutrition. My point, however, is that the human body can store large amounts of fat and expend them when necessary. We may use as an example the water supply of a modern city like New York. The city's consumption of water is not increased after each heavy rain, nor is it diminished in periods of drought until the reservoirs are seriously depleted. All of which leads us to the conclusion that there is no need of fussing about small changes in the patient's dietary.

The question of the mineral salts in our dietary has recently assumed a good deal of importance. Sodium chloride has been blamed, not only for the production of edema in cardiac and renal diseases, but has also been accused of contributing materially to hypertension. At first, attention was focussed almost exclusively on the chlorine portion of the sodium chloride molecule, and a diet low in table salt is often referred to as "a chloride poor diet." This has fixed in the minds of the medical public dangers attributed to chlorine. More recently, biologists and physiologists have been putting the blame on the sodium content of table salt, and it must be confessed that their evidence is fairly strong. Clinicians on the whole have been rather disappointed by the effects of salt reduction on the edema of cardiac and renal disease. Occasionally withdrawal of salt relieves an edema, but much more striking effects are often obtained with calcium chloride, especially if supplemented by salyrgan. It is, however, necessary to issue a note of warning, because the general condition of a patient does not necessarily improve in proportion to the disappearance of the edema.

The question of the rôle of table salt in the causation of hypertension is still unsettled. Allen is quite emphatic that hypertensive patients are benefitted by salt reduction, and he ascribes the failures of others to the fact that their diets have not been strict enough and have not been continued for sufficiently long periods. In Germany, a salt poor diet in tuberculosis has been urged as a method of decreasing the water content of the tissues and as an aid to calcification. Salt withdrawal is a part of the Gerson-Hermannsdorfer-Sauerbruch régime. There has been a large amount of clinical observation on this subject but most of it has been rather loosely controlled and the question is not yet settled.

From the fruit-growing state of California comes an appeal for an alkaline ash diet as a cure for hypertension and many of the other ills of life. Most of us are getting along with urine that is distinctly acid on account of the preponderance of cereals and meat in our diet. If we would only change to a diet with little meat and cereal and a large proportion of beans and citrous fruits we would have alkaline urine. Oranges, lemons and grapefruit may taste acid but the citric acid is burned, leaving the basic salts to neutralize other acids in our bodies. From Germany, however, comes a plea for an acidification of the diet if we wish to cure tuberculosis. There is a great deal of talk but very little evidence. At the present time we should be careful not to be carried away by propaganda.

Calcium is deservedly coming in for a great deal of attention. Sherman has shown that the American diet is dangerously near the minimal calcium intake theoretically compatible with health. The discovery of parathormone and viosterol has given us means of modifying the calcium metabolism. In certain definite diseases of calcium metabolism the clinical results have been striking. These two products sometimes need to be supplemented by a diet rich in calcium and for this indication milk has shown its value. Calcium lactate is being used extensively and good results

have been obtained with calcium gluconate. When it comes to the question of the diet for diseases not definitely associated with the disturbance of the calcium metabolism, we are still without evidence as to the optimal calcium intake.

This naturally brings us to a discussion of the vitamins. There is no question at all as to the value of the vitamins in cases of well-established vitamin deficiencies. In patients who have definite symptoms of these deficiencies, the results of feeding the proper vitamin are almost miraculous. Here, in New York, frank cases of vitamin deficiency are comparatively rare. Even at Bellevue Hospital where we treat the poorest classes of the city, and care for many sailors, many foreigners ill-adapted to our New York life, many down-and-outers who are either densely ignorant or mentally subnormal, and many chronic alcoholics, we have relatively few clear-cut cases. I, myself, have never seen xerophthalmia, or beri-beri. We often go months without finding an adult case of scurvy. Osteomalacia is extremely rare. Pellagra in its more severe manifestations is uncommon, in New York, but we do observe among chronic alcoholics quite a few men with the milder symptoms. These patients usually give a history of having subsisted for many weeks on a diet of coffee and doughnuts.

What about the effects of vitamins in patients who do not show the frank symptoms of deficiency? Should we deliberately strive in the case of healthy persons and with most of our patients to provide a diet containing a much larger amount of vitamins than has been the custom here in New York? It is very hard to obtain direct evidence on this point. The poor of this city probably take in their diets a small fraction of the amount of vitamins consumed by the rich. They seem to be somewhat more susceptible to disease, but not markedly so, and it is only when we get down to the lowest levels of poverty, shiftlessness, alcoholic indulgence, and exposure that we find a sharp increase in the mortality in such diseases as lobar pneumonia and tuberculosis. We must remember, however, that many factors besides vitamins affect this situation.

The Mellanbys believe that there are substances in some foods, such as oatmeal, that act in a manner opposed to the vitamins, and they believe also that an excess of vitamins will increase resistance to infection and prevent decay of the teeth. There is a good deal of evidence that a diet rich in a variety of vitamins, particularly A, C, and D, will increase resistance to tuberculosis and help in the prevention or cure of intestinal lesions of this disease. There are many reports indicating augmented protection against minor infections. The widespread popular movement towards a more liberal vitamin ration is most cheerfully aided and abetted by the manufacturers of every foodstuff containing more than a trace of vitamin. Probably we should allow ourselves to be carried a moderate distance by this wave of popularity. A diet high in vitamins is apt to taste good, seems to improve the appetite, and usually carries with it an increase in the mineral salts. There is only small evidence that such a diet can do harm. Very large doses of vitamin D, as contained in high-potency viosterol, are dangerous, and it is quite possible that any large amounts of one vitamin unbalanced by others may produce untoward effects.

When it comes to the practical administration of the vitamins, we must remember that they vary greatly in their responses to cooking, drying, and storage. Vitamin A, the fat-soluble vitamin which cures xerophthalmia and aids in growth, is fairly stable to heat and is only slightly affected by cooking or by canning, if air is excluded in the process. Vitamin D is very resistant, as is vitamin B₂, sometimes called vitamin G, the one that is associated with the prevention and cure of pellagra. Vitamin B₁, sometimes called F, or the antineuritic vitamin, is partially destroyed by heating to a high temperature in the presence of alkali; vitamin C, the antiscorbutic vitamin, is rapidly destroyed by cooking in an alkaline or even in a neutral solution, and is also susceptible to drying or aging. Fortunately, one of the best sources of this vitamin, tomato juice, is not spoiled by careful cooking, or canning, because it is acid.

There has been a widespread prejudice in this country against canned vegetables, and a great many people still believe that the so-called "fresh vegetables" cooked in the home contain more vitamins than canned foods. This is probably not the case, as the home-cooking of vegetables, or fruits, in an open vessel is an excellent method of oxidizing the vitamins, particularly if cooking soda is added to preserve the green color and improve the flavor. Most of the canners are now taking every precaution to avoid the oxidation of the vitamins. Fruits and vegetables are prepared when they are very fresh, and if they are to be puréed they are hashed in an atmosphere from which oxygen has been excluded and cooked in one that is practically oxygen-free. In all products submitted to the Committee on Foods, tests on animals are made which show as quantitatively as is possible at the present day the content of the vitamins.

The importance of roughage in the diet has been the subject of a good deal of controversy. In Germany, there has been great enthusiasm for a raw diet which is high in vitamin content and high also in indigestible residue. Some of the reports say that it is borne surprisingly well, but most of the German physicians seem to look on it with mild amusement. Again, in California, there is some tendency toward a diet with a very bulky residue. Alvarez, of the Mayo Clinic, believes that an excessive amount of roughage can do a great deal of harm, and has obtained with a bland non-residue diet results that would delight the strongest advocate of bulky residues. Bran has had a tremendous vogue among the laity, encouraged by the advertising of manufacturers of bran products. Recently, I canvassed the best gastroenterologists that I know for their opinions and found that none of them advised the use of bran, although they did not seem to think that small amounts were particularly harmful. The excessive quantities of bran and other roughage consumed by some individuals with a frantic desire to cure constipation may do a great deal of damage. Man was not given the digestive system of the ox.

We have just reviewed a large number of debatable points in the dietary of disease. How can we secure reliable evidence as to whether or not a certain diet is of benefit? Clinical impression is the popular answer, and it would seem fairly satisfactory were it not for the fact that clinical impression, in past decades, has strongly backed about one thousand drugs or diets now discarded, for every dozen drugs or diets now employed. There is no good reason why a diet that is worth while should not give results that are definite enough to become apparent when treated in a perfectly scientific manner and subjected to analysis. Suppose, for example, we wish to prove that a certain diet in lobar pneumonia is better than the conventional one used in this disease. The first thing to do would be to assign alternately our pneumonia cases to two groups, one receiving the new diet, the other, the old. Care would have to be exercised in order that each group should contain approximately the same proportion of pneumonias of the severer types, etc. Judging from my past experiences in testing serum and digitalis in lobar pneumonia, I should say that we would probably have to collect at least four hundred cases on the new and four hundred on the old before we could say definitely that the new diet produced a lower mortality than its predecessor. If the results were very striking and one group showed a mortality of 40 per cent, while the other group showed a mortality of 30 per cent, we could perhaps obtain our answer before we had studied 800 cases. Of course, a much smaller number of cases carefully studied would give us information regarding the effect on abdominal distention, nitrogen balance, etc. In the case of typhoid fever, Coleman was able to establish the beneficial effects of his diet fairly definitely by an analysis of 111 cases on a high calory diet, as opposed to about 773 controls on a previous low calory régime. I had the good fortune to be associated with Dr. Coleman in part of this work, and I know the careful studies that supported the relatively small number of cases from which the final conclusions were drawn. Shaffer and Coleman spent several years in a study of the nitrogen balances. I, myself,

spent about two years in a study of the absorption of the food in his cases. Dr. Coleman and I studied the caloric requirements and the effects of food on metabolism, first, for two years with a small respiration apparatus, and then for two more years, in a respiration calorimeter. Torrey made an extensive investigation of the bacterial flora. Finally, the experiences of many other clinics confirmed Coleman's conclusion. None of this work was wasted—it was all necessary to prove the point.

Let us contrast with this prolonged study the methods that have been used in forwarding the Gerson-Hermannsdorfer-Sauerbruch diet in tuberculosis. This diet, first devised by Gerson, is high in vitamins, sodium chloride is excluded as much as possible, and a mixture called mineralogen is included, consisting of a dozen salts, the main constituents being calcium phosphate and calcium lactate, supplemented by other compounds, one of which is a substitute for salt, the other probably contains cod liver oil and phosphorus. This diet has been given to a large number of tuberculosis patients; some clinicians are very enthusiastic about its effects in lupus, and others say that it is of great help in bone tuberculosis and in the healing of wounds. In certain quarters its results in pulmonary tuberculosis are highly praised, but a good many writers insist that this régime possesses no advantage over the customary diet which contains liberal amounts of vitamins. What would be required to put this régime to a really scientific test? I believe that it would be necessary to select two hundred patients with tuberculosis, and, as controls, an equal number with approximately the same localization and severity of infection. These two groups should be followed for one or two years before drawing any conclusions. If by that time the régime showed a significant advance in therapy, it would be essential to test out the various factors, one at a time, in order to know whether it was a high vitamin content, or a low sodium chloride, or a complicated mixture of salts that was responsible for the improvement.

I suppose it is too much to hope that diets for diabetes will be tested in this manner. In the last eighteen years I have watched many diabetic diets come and go and each time that a new one has become fairly well-established I have wondered how long it would last. During this period, we have had the Naunyn diet, the Allen treatment, the Shaffer-Woodyatt balanced diet, the Newburgh and Marsh high-fat diet, the insulin régime with increasing carbohydrate, and now we have reached an era when the carbohydrate is being raised still higher in our effort to make the patient happier and to prevent the possible development of arteriosclerosis. There is much to be said in favor of giving liberal amounts of carbohydrate if the insulin is carefully regulated, but this diet is not an easy one for the doctor, and its use requires a great deal of skill. In fact, every diabetic diet requires skill and a fairly extensive knowledge of the fundamental principles of metabolism. I think we should remember that the basis of an excessively high carbohydrate diet for patients who require insulin is largely theoretical, and that it is going to be many years before statistics will show whether or not we have diminished the incidence and severity of arteriosclerosis by this treatment. Personally, I am more inclined towards a moderate carbohydrate intake and smaller doses of insulin. Still, there are a great many things that we do not know about carbohydrate metabolism. There is a good deal to show that if the carbohydrate intake is restricted too much the patient loses tolerance, and that carefully adjusted increases in the carbohydrate ration may lead to an increased ability to oxidize this food-stuff. For many years we strove to protect this oxidative function by keeping the demands upon it as low as possible. Now we strive to exercise it judiciously but in our enthusiasm for the new method we must be careful not to overstrain this function. Experience has shown that when a diabetic indulges in carbohydrate excesses he pays the penalty with diminished tolerance and perhaps even with coma.

I wish that it were in my power to state exactly the

best diet to be used in obesity. All we know is that if you want to reduce the weight of a patient you must see that the caloric intake is distinctly less than the caloric output. If this is kept up for over a month and the patient does not happen to develop an edema, there is no way that he can escape losing weight unless he finds some supernatural means of violating the law of conservation of energy. It is curious how many doctors adopt theories regarding obesity that violate this fundamental law, when their sole evidence against it consists in statements from their patients to the effect that they are not large eaters. Newburgh and Johnston have recently published reports of careful studies which show that obese patients who are carefully watched and given sub-maintenance diets always lose weight, although such loss may be masked for one or two weeks by a retention of water in the body. This finding is by no means new, but these workers have demonstrated it most convincingly. They have emphasized the old belief that obesity is caused by too much food in relation to the expenditure of energy and they disregard the various complicated classifications of obesity that have confused our literature. All clinicians agree that there is a form due to over-eating, but they also set apart groups of patients in which the trouble is supposed to lie in one or more of the ductless glands. It is in these groups that they seek for supernatural mechanisms. It may be true that in a given patient with hypothyroidism, or dyspituitarism, the basal metabolism may be lower than normal and there may perhaps even be a greater economy in the digestion of food and the performance of muscular tasks. In such a patient the total expenditure may be low, but if the intake is still lower a loss of weight is inevitable. There has been an extensive search for the mysterious factors that border on the supernatural. A good many European and some American investigators have reported that the specific dynamic action of foods is reduced in obesity. McClellan and Spencer, in our laboratory, and a good many workers in other laboratories have been unable to confirm this fact. I do not think that any significant abnormality

has been found in the metabolism in obesity except an increased ability to metabolize fat without ketosis. Obese patients have an unusual tolerance for a low carbohydrate diet. Although they tolerate a low carbohydrate diet this does not necessarily mean that such a diet is indicated. In our reducing diets we should provide enough protein to maintain a patient in nitrogen equilibrium, which can probably be accomplished by almost any amount over 60 grams a day. I do not know of any convincing evidence as to whether we should provide the remaining calories chiefly in fat or chiefly in carbohydrate, so perhaps it is just as well to leave this to the individual doctor who will quickly enough form a strong prejudice for one or the other.

It is impossible in this brief talk to mention more than a few of the diets used in the clinic. There is, however, one point of fundamental importance that I should like to emphasize and that is the time factor. If a diet is planned for a long period, it is essential that it should contain all the calories and individual elements necessary for adequate nutrition, unless we are deliberately attempting to reduce weight by restricting calories. If a diet is to be used for only a few weeks, as, for example, the strict Sippy diet in peptic ulcer, we can relax our vigilance regarding the various constituents, even some of the vitamins. Healthy men can starve for a month if given plenty of water. Patients with fever and toxemia do not tolerate starvation nearly as well, but, nevertheless, they can draw on their bodies for many pounds of fat and of protein and for considerable amounts of calcium and other salts. A man who has been well fed can begin his starvation with a reservoir of about a pound of carbohydrate and several liters of water. Now these reservoirs are relatively small and they are the ones on which the clinician should concentrate his attention.

In a short and stormy illness where complete loss of appetite, or nausea, has limited the food intake, we do not need to worry much about the protein and the fat but we

must remember that it only takes two or three days to exhaust almost completely the small glycogen stores of the body and also the small reservoirs of water. Acute illness is usually accompanied by high metabolism which uses up carbohydrate rapidly. Fever and high metabolism lead to excessive losses of water through skin and lungs. If at the same time the fluid intake is diminished there will be a serious dehydration of the tissues, especially if there has been vomiting, diarrhea, or diuresis. I do not think it is too much to say that the dietetic error that has killed more patients than any other is the neglect of administering enough water. If water cannot be given by mouth, it must be given by rectum, by vein, or by the subcutaneous method. The practitioner should remember two things, first, that patients lose more than 600 grams of water a day through skin and lungs, and, second, that most people ingest about half of their water in the form of their so-called "solid foods," particularly such foods as lettuce, tomatoes, fruits, potatoes, etc. When these solid foods are cut off in disease it is necessary to supply a proportional increase in the liquid foods in order to bring the water intake up to the level of that of the ordinary man in health. In acute disease, we should make a still further increase in order to compensate for the excessive loss of water through skin and lungs. I beg the practitioner who is taking care of a patient severely ill to watch the urine and see that it does not become too concentrated. A high colored urine is a reproach to the doctor and to the nurse. A dehydrated skin and tongue are danger signals.

When you try to feed patients who are acutely ill, the limiting factor is usually lack of appetite. How I have longed for some hormone that would bring the appetite just to the desired level, so that the patient would take gladly the food that is offered to him. We should not be too placid in our acceptance of this lack of appetite. Often when a nurse has told me that a new patient, say with typhoid fever, has no appetite, I have found the tongue dry and cracked and the teeth covered with sordes. How could any man have an appetite with a mouth of that sort?

Why is his mouth in that condition? Often it is dry because he is breathing through it, and he is breathing through it because his nose is stopped with dried mucus. Therefore, if you want to combat in a typhoid patient the phenomena of a low respiratory quotient, ketosis, negative nitrogen balance, etc., you should start with the simple operation of clearing out the nose so that the patient can breathe through it. You should next clean his mouth as rapidly as it can be done without undue trauma. You should then bring back the appetite by forcing food until the patient gets sufficient strength to have some appetite of his own. In the course of a few days you will often be surprised to find that a patient in the midst of his typhoid fever, will have teeth and tongue as clear as your own.

If we only had the proper hormones to control appetite how easy would be our task. We could bring the undernourished man up to the ideal weight; we could give the diabetic the proper combination so that he would not desire to steal sweets; we could give the glutton an anti-hormone so that he would no longer want to eat too much. Let us look forward to the day when we can turn to the office nurse and say, "Give this patient a 1500-calory appetite."

In conclusion, I would like to speculate on the possibilities of dieto-therapy. Many enthusiasts believe that proper diets will cure practically all our ills, except a few that require surgery. I hate to be pessimistic but I cannot believe that diet will ever become of very much more importance than it is at the present time. The enthusiast points to the recent advances made through diet in the treatment of diseases of vitamin deficiencies and of pernicious anemia. Far be it from me to belittle the importance of these discoveries, but are they really dietary? The biochemists, with the aid of the large drug houses, have taken these food-stuffs and concentrated them until the active principles are now contained in pills. Shall we call liver extract a drug or a food? No one at the present time

considers thyroid extract a food, but a man who called himself a "dieto-therapeutist" might administer to his myxedema patient a hash containing thyroid extract; he could treat his patients having constipation with a stew made of senna leaves and figs, and malaria with a bitter tea made from cinchona bark. So, after all, it does not make much difference what we call these various agents, as long as we recognize their true natures and utilize them intelligently.

Not only must we recognize the true natures of the agents but we must also recognize their limitations. We must scrutinize with care all of our evidence. We must differentiate clearly between traditions founded on impressions and facts based on scientific observations. In each individual patient we must visualize the various metabolic processes and must direct our therapeusis to meet these specific indications.
